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MORE GUNS, MORE UNINTENDED CONSEQUENCES:
THE EFFECTS OF RIGHT-TO-CARRY ON
CRIMINAL BEHAVIOR AND POLICING IN US CITIES

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ABSTRACT

We analyze a sample of 47 major US cities to illuminate the mechanisms that lead Right-to-Carry concealed handgun laws to increase crime. The altered behavior of permit holders, career criminals, and the police combine to generate 29 and 32 percent increases in firearm violent crime and firearm robbery respectively. The increasing firearm violence is facilitated by a massive 35 percent increase in gun theft ($p=0.06$), with further crime stimulus flowing from diminished police effectiveness, as reflected in a 13 percent decline in violent crime clearance rates ($p=0.03$). Any crime-inhibiting benefits from increased gun carrying are swamped by the crime-stimulating impacts.

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More Guns, More Unintended Consequences: The Effects of Right-to-Carry on Criminal Behavior and Policing in US Cities

By JOHN J. DONOHUE, SAMUEL V. CAI, MATTHEW V. BONDY, AND PHILIP J. COOK

In 2019, police agencies in the United States reported nearly 300,000 aggravated assaults, robberies, and homicides committed with a firearm (Federal Bureau of Investigation, 2020). At the political epicenter of this social problem is the legal right of individuals to carry concealed handguns in public. Many economists, beginning with Lott and Mustard (1997), have attempted to measure the effect of Right-to-Carry (RTC) concealed handgun laws on violent crime (see Smart et al. (2020) for a serviceable review of the early literature on the topic). While much of this early literature found no statistically significant effects of RTC laws on crime rates (Dezhbakhsh and Rubin, 1998; Ludwig, 1998; Black and Nagin, 1998), the predominant conclusion from studies in the last five years has been that RTC laws increase violent crime.¹ This more recent literature has benefited from 1) more complete data, which enables the use of longer panels with greater treatment variation as more states have passed RTC laws, 2) advances in policy evaluation, such as improved econometric techniques in model selection and standard error estimation, and 3) the development of new tools such as synthetic controls (Donohue, Aneja and Weber, 2019) and Bayesian methods (Schell et al., 2020).² Using a state-level panel from 1979-2014, Donohue, Aneja and Weber (2019) estimated that RTC laws increased violent crime by 9.02 percent ($p < 0.01$) and provided event-study analyses that both supported the critical parallel-trends assumption and illustrated that the upward trend in violent crime began in the year following adoption of the RTC law. Since we now have five additional years of data beyond the 1979-2014 data period used by Donohue, Aneja and Weber (2019), we begin by running this same state-level panel data model over the 1979-2019 period. The results are virtually identical: the static estimate shows that RTC laws increase violent crime by 9.25 percent ($p < 0.01$), and the event-study analysis shown in Figure 1 again highlights the validity of the model and buttresses the causal finding that RTC laws elevate violent crime.

While the weight of the most advanced research on RTC laws now supports a finding that such

¹See Table 1 of Donohue (2022) summarizing 14 recent papers that reach this conclusion.

²Schell et al. (2020) find that firearm homicides increased one year after implementation of RTC laws with probability of .99, but suggest that this effect weakens over time.

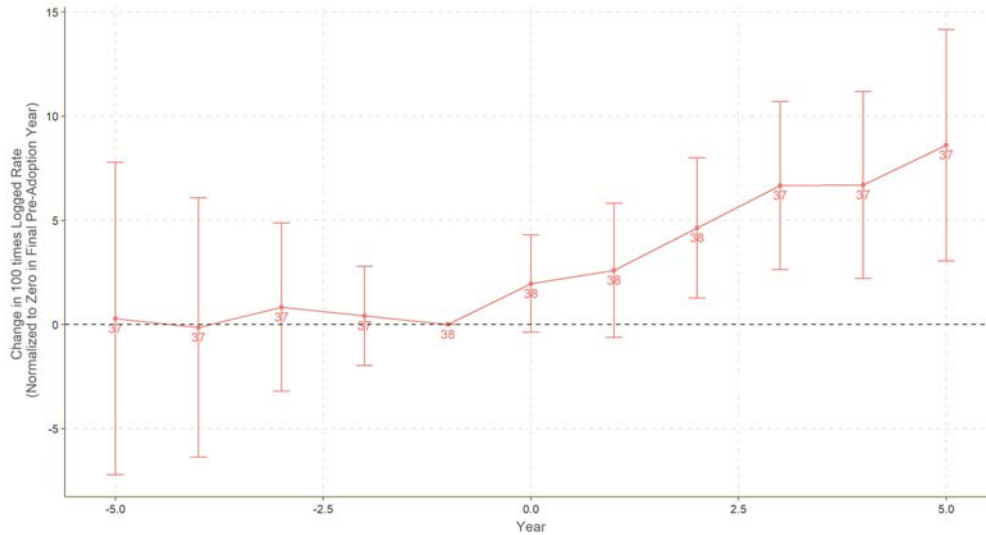


Figure 1. : The Impact of RTC Laws on Violent Crime, Donohue, Aneja and Weber (2019) model, 1979-2019.

Note: We regress crime on dummies for pre-and post-passage years and DAW covariates. Year 0 represents the first year with RTC in place at any time, meaning that in states that adopt after January 1, this will capture only a partial effect of RTC laws. We display the 95 percent confidence interval for each estimate using cluster-robust standard errors and show the number of RTC jurisdictions that contribute to each estimate.

Source: Author-cleaned data set.

laws generate net increases in violent crime, there has been relatively little work clarifying the mechanisms that lead to this outcome. Donohue, Aneja and Weber (2019) discussed a number of plausible pathways, ranging from increased violence and road rage by permit holders to diminishing police effectiveness as officers became less able or willing to engage in crime-suppressing activity after RTC adoption (see also Fridel, 2021). In addition, Donohue, Aneja and Weber (2019) drew upon survey data from Hemenway, Azrael and Miller (2017) to estimate that the increased gun carrying in the wake of RTC adoption would lead to an additional 100,000 guns stolen from permit holders each year. Additionally, Billings (2020), using rich data from a single county in North Carolina concluded that RTC laws facilitate gun thefts that stimulate violent crime increases. Of course, there can be crime-reducing benefits from gun carrying by private citizens if crimes are thwarted or deterred and criminals are captured or injured from defensive gun use, but if the current weight of the evidence is correct any such benefits are substantially offset by the crime-enhancing impacts of increased gun carrying. It is these pernicious mechanisms that this paper attempts to clarify and measure.

To this end, we exploit differential timing in the adoption of RTC to examine the effect of concealed carrying on the rates of and mechanisms driving firearm and nonfirearm violent crime in America's largest cities during the period from 1979 to 2019. We introduce an author-cleaned

dataset derived from agency-level Uniform Crime Reporting (UCR) and Supplementary Homicide Reports (SHR) records and perform a difference-in-differences (DiD) analysis to estimate the impact of RTC laws on firearm and non-firearm violent crimes, clearance rates (the rate at which police are able to identify and arrest the perpetrators of crime), and the monetary value of stolen guns.³

We present evidence to suggest that the mechanisms of greater levels of gun loss and theft and reduced police effectiveness are potent drivers of increased crime following RTC adoption. We find that RTC laws cause a roughly 13 percent decline in the rates that police clear violent crime, suggesting that RTC laws strike at the very heart of law enforcement's abilities to address criminal conduct. Moreover, this significant decline in police clearance rates is not simply a product of RTC laws encouraging more crime, which implies that the strains on police are due to more than simply an increase in the number of crimes. Even when we control for this increased violent crime rate, RTC laws still dampen the rate at which violent crime is cleared. We also find that the introduction of an RTC law elevates gun thefts by roughly 35 percent, introducing tens of thousands of guns into the hands of criminals or illegal gun markets each year.

We also show RTC laws cause statistically significant increases in crime, thus indicating that the summation of these two (and other) criminogenic effects unambiguously dominate any crime-reducing benefits derived from criminals' perceived increased probability that they will meet armed resistance while committing a crime or from actual incidents of effective defensive gun use. The rate of firearm violent crimes rises by 29 percent due to RTC, with the largest increases shown in firearm robberies. We address issues regarding the robustness of our findings to threats from heterogeneous treatment effects, non-parallel pretrends, and other DiD assumptions using Goodman-Bacon (2021) decompositions and event-study analyses.

Our paper offers several contributions. First, we illustrate that widespread gun carrying has many implications for the dynamics of crime that go beyond whether a permit-holder can defend against crime or uses a gun to cause harm in a moment of anger. Indeed, the effects of RTC laws that we are able to document most precisely—increased gun thefts and diminished police effectiveness—are largely caused by criminogenic mechanisms unleashed by even law-abiding concealed carry permit-holders.⁴ This paper is the first of which we are aware that provides credible causal estimates of the effects of RTC laws on gun theft or policing. Situating our findings within the Becker (1968)

³Note that in discussing our analysis and results, we make the simplifying assumption that the monetary value of guns stolen is directly proportional to the number of guns stolen.

⁴Recognizing the dangers to the public from stolen guns, Israel imposes substantial criminal penalties, including jail time, for negligent storage of weapons resulting in theft, which would include the common practices of many American RTC permit holders (and gun totes in permitless carry states) that elevate the risk of gun theft, such as leaving firearms in unlocked cars.

crime model, we provide both a theoretical and empirical basis for understanding these effects.

Second, our focus on major cities, where violent crime is most concentrated, stands in contrast to the vast preponderance of the existing literature, which has been focused on state-level and county-level panel data for the last several decades. County-level crime data have been shown to be marred by problematic imputation practices (Kaplan, 2021*e*). Kovandzic, Marvell and Vieraitis (2005) were among the most recent to examine the causal effect of RTC across major cities in the US.⁵ The study covered the period from 1980 to 2000 and used a linear spline model to identify the effect of RTC laws on trends in violent crime.⁶ Our current study, which relies on agency-level UCR data from 1979 to 2019, uses better quality data covering a longer time period, and presents findings on overall average treatment effects as well as event-study analyses.

Finally, we seek to make a methodological contribution to the empirical literature through the application of rigorous new techniques of robustness testing for panel data policy analysis. We provide event studies to consider concerns regarding potential violation of parallel trends as well as Goodman-Bacon (2021) decompositions of our regressions into individual 2x2 treatment vs. control comparisons to 1) demonstrate the robustness of our findings to heterogenous treatment effects and 2) consider the validity of our proposed counterfactual by performing weighted covariate balance testing. Our application of the insights gained from recent DiD econometric scholarship reinforces a large body of research finding that RTC laws elevate violent crime.

The remainder of this paper is organized as follows. Section I presents a brief overview of concealed carry laws in the US. Section II presents the theoretical motivation for our study using the Becker (1968) economic model of crime. Section III describes the data used to complete our main empirical analysis. Section IV contains our empirical estimates. Section V discusses how our results conform to the outcomes predicted within our theoretical model and to the existing literature on the economics of crime. Section VI concludes.

⁵In their literature review, Smart et al. (2020) also note the findings of La Valle and Glover (2012) and La Valle (2013), but we find several serious flaws in their coding of the adoption of RTC laws that lead to the results being unreliable as estimates of the true effect of RTC laws on crime in US cities.

⁶KMV essentially found that each component of violent crime rose by roughly 1 percent per year following RTC adoption, except for aggravated assault which grew at twice that rate (and was the only one of the four estimates that was statistically significant). The specific estimated annual increment to crime for the four crime categories (with t statistics in parentheses) were 1.1 percent (0.80) for homicide, 1.0 percent (0.91) for robbery, 1.2 percent (1.33) for rape, and 1.9 percent (2.59) for aggravated assault, in their main specification. Regressions were “weighted by a function of population as determined by the Breusch-Pagan Test.”

I. Background

The United States has undergone a major shift in gun policy over the last four decades. In 1980, the vast majority of American states either banned the concealed carry of firearms or had laws that required someone who wished to carry a gun to apply for a permit and perhaps justify a need to do so or establish that the individual was a person of good character. Most states that initially allowed concealed carry tended to have may-issue laws, so called because they stipulated that authorities “may issue” concealed carry licenses under certain conditions, but did not guarantee it as a basic right. In contrast to may-issue regimes, RTC laws require that the state “shall issue” a license to any individual who requests one with certain narrow exceptions, such as those with felony convictions or underage individuals. For example, from 1870 to 1995, Texas banned carrying of firearms outside the home, with few exceptions. This applied to both openly carried and concealed firearms (Rivas, 2019). In 1996, it transitioned to a RTC regime, thereby allowing concealed carrying of handguns, and in 2015 it endorsed open carry of firearms. Finally, in September 2021 the state adopted “permitless carry” allowing all citizens older than 21 to carry firearms without a license.⁷

An extensive and highly successful gun lobby campaign designed to promote lagging gun sales gradually encouraged most states to adopt RTC laws. The gun lobbying campaign to promote RTC laws enlisted gun enthusiasts who believe the right to carry was a “principle of individual right, of personal honor” (Patrick, 2010) that could be advanced by promoting an individualistic interpretation of the Second Amendment (Charles, 2018). Politicians who supported expanding the right to carry often cited the Second Amendment as their rationale (Marley and Glauber, 2011) or asserted that these laws could protect civilians from crime.

This last factor raises a possible difficulty for researchers if RTC adoption is endogenously linked to rising crime rates. However, the systematic review of the role of RTC laws in affecting crime that is now frequently cited in legislative hearings and court cases did not begin until the late 1990s, by which time most states who would adopt RTC laws had already done so. Furthermore, RTC adoption came via different paths for different states (either court ordered or legislatively mandated), often only after repeated failed attempts, and was driven by a relatively small group of highly politically active progun operatives and citizens (Patrick, 2010). Additionally, the RTC laws present in the cities in our sample were adopted at the state level, suggesting that forces driving the passage of state RTC laws were at least one step removed from the forces driving city crime trends.

⁷Donohue, Aneja and Weber (2019) analyzed the statutory history of RTC laws in conjunction with an extensive search of newspaper archives to determine the dates of concrete changes in concealed carry law through 2014. We update this data to reflect relevant legal changes since 2014.

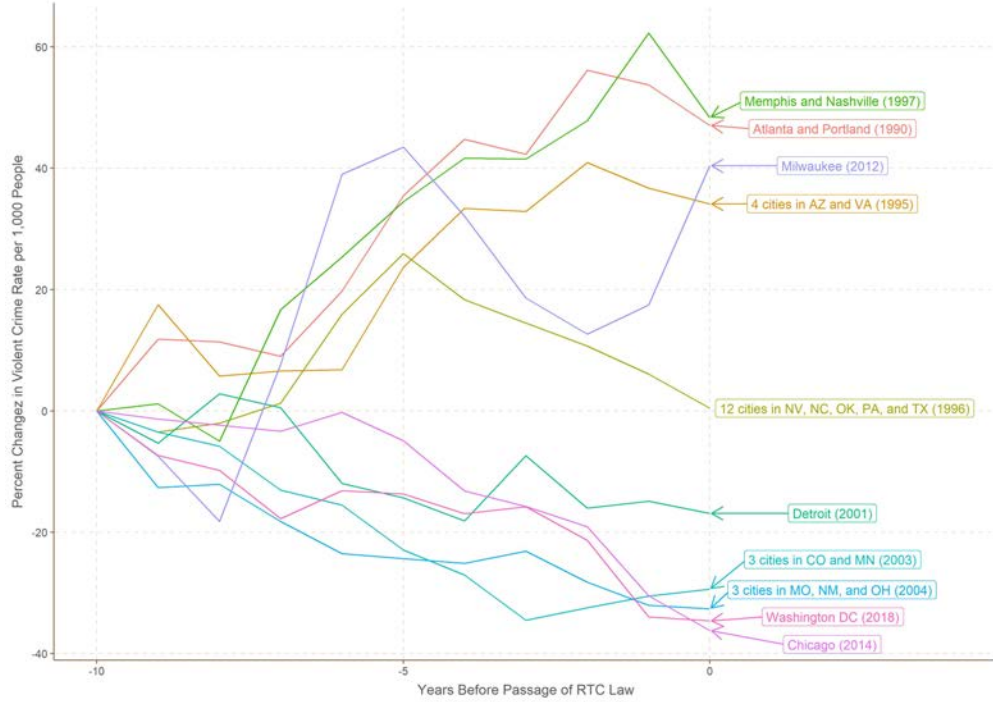


Figure 2. : Trends in Violent Crime in 30 RTC Cities Prior to Passage of RTC Law

Note: Crime rates are shown as a percent change from the 10th year prior to RTC passage. Cities that pass RTC laws in the same year are collapsed into a single population-weighted unit for clarity. The 1996 group consists of: Austin, Charlotte, Dallas, El Paso, Fort Worth, Houston, Las Vegas, Oklahoma City, Philadelphia, Raleigh, San Antonio, and Tulsa. Seattle (1961), Indianapolis (1980), Jacksonville (1988), Miami (1988), Louisville (1997), and Omaha (2007) are omitted due to lack of sufficient data.

Source: Author-cleaned data set.

Indeed, Figure 2 visually shows that violent crime rates do not follow any noticeable pattern in cities in the ten years prior to RTC adoption. Thus, we conclude that the adoption of RTC across cities in our sample can be considered reasonably exogenous to the outcomes we study. In our section on robustness, we produce event-study plots and perform qualitative tests of conditional parallel pre-trends to further justify our empirical strategy.

II. Theoretical Motivation

We draw insight from the Becker (1968) crime model to consider theoretically how a rational criminal may react to the passage of RTC laws.⁸ Becker models the expected utility of an individual

⁸Note that the Becker model is not without its drawbacks, as it does not provide a direct basis for understanding the *rate* of committing crime. The framework models a single individual making a yes/no decision about whether to commit a crime, but in reality potential criminals choose from a rich environment of possibilities. Further, the model does not consider how committing crimes in the past affects the decision to commit a crime in the present, which Mocan, Billups and Overland (2005) have tried to overcome in developing a dynamic economic model of crime. Nevertheless, we inform our discussion of the mechanisms by which RTC laws influence crime using the Becker model because its parsimonious nature enables us to provide some useful predictions that find support in our empirical analysis.

considering crime as

$$(1) \quad EU = pU(Y - f) + (1 - p)U(Y)$$

where p represents the probability of getting caught, Y represents the income associated with committing a crime,⁹ and f represents the average sanction associated with getting caught committing a crime. For our analysis, we make some small modifications to Becker's model: we define income as net income after costs, and we also split f and p into two separate sanctions and probabilities: f_j represents legal sanctions (like jailtime), while f_s represents the sanction of being shot at by a firearm (and possibly injured or killed) while committing a crime, and $p(j)$ and $p(s)$ represent the probabilities of each of these events respectively. Thus, we redefine expected utility of crimes as¹⁰

$$(2) \quad EU = U(Y) - p(j)U(f_j) - p(s)U(f_s)$$

Using this framework, we consider the expected utility of crime before and after an RTC regime. Let variables denoted with ' signify that they refer to *post-RTC implementation*. Then the change in expected utilities due to an RTC regime can be modeled as

$$(3) \quad EU' - EU = \underbrace{U(Y' - Y)}_1 - \underbrace{[p'(s) - p(s)]U(f_s)}_2 - \underbrace{[p'(j) - p(j)]U(f_j)}_3$$

Our following analysis considers the deterrence effect and criminal response to RTC laws, and identifies under what conditions $EU' - EU > 0$ (when expected utility grows under an RTC regime).

The easier access to guns that RTC laws enable can trigger both stimulants to and constraints on crime. RTC laws can suppress overall violent crime by deterring, thwarting, or incapacitating criminals due to the enhanced risk of attack from an armed victim, which increases the cost of crime. Within the above framework, if an RTC law raises the probability of adverse outcomes for criminal actors, $p'(s) > p(s)$, and so Term 2 will reduce the net utility of crime under a RTC regime, all else equal. An unintended consequence of the enhanced potential danger from permit holders, however, may be that criminals arm themselves in response to this increased perceived

⁹As Becker notes, this income can be both "monetary and psychic."

¹⁰Suppose that $p(s)$ and $p(j)$ are independent. Then we can decompose the expected utility into the four distinct possibilities, $(1 - p(j))(1 - p(s))U(Y) + p(j)(1 - p(s))U(Y - f_j) + (1 - p(j))p(s)U(Y - f_s) + p(s)p(j)U(Y - f_s - f_j)$, which simplifies to Expression (2) if one assumes linear addition of utilities, e.g. $U(f_j) + U(f_s) = U(f_j + f_s)$.

threat, elevating the proportion of crime committed with a firearm. Additionally, there are at least two other mechanisms tied to the criminal response to RTC laws that may have damaging criminogenic effects.¹¹

First, RTC laws may decrease the cost of obtaining firearms for criminals. We refer to this mechanism as the “facilitation effect.” Billings (2020) establishes a strong link between RTC permits leading to increased gun thefts using 2007-2011 data from Charlotte, North Carolina. Increased legal carrying of firearms outside the home may lead to more opportunities for gun theft, particularly from vehicles (Elinson and McWhirter, 2022). The increased supply of illegally obtained firearms would be expected to decrease the price of illegal firearms on the informal market, thereby facilitating illegal firearm use even beyond the original gun thief. Lowering the cost of committing a firearm crime would increase the net income of committing a firearm crime (i.e. $Y' > Y$ for firearm crimes), and have no effect on the utility of non-firearm crimes. It is of note that only 40 percent of robberies are committed with a firearm in our sample, despite the fact that firearms greatly increase payoff to robbery by increasing the probability of “success” and allowing criminals to pursue more robust targets like commercial establishments (Cook, 1987, 1991). This suggests that firearms are scarce to robbers, and that the availability of firearms is an important margin in criminals’ decision-making on whether to pursue more lucrative firearm crimes. Additionally, RTC laws may decrease the risk of carrying an illegal firearm, as police may be less proactive about searching individuals for illegal firearms.

Second, increased gun carrying by citizens will increase crime if it impairs police effectiveness in any of a number of ways.¹² RTC laws may generate a host of demands on police time and resources that reduces the amount of time they have to fight crime. Processing complaints about the increased gun thefts, accidental discharges and injuries, processing RTC permit applications, and taking time to check for permit validity by those carrying guns will all encumber police resources (Donohue, Aneja and Weber, 2019). Additionally, police officers may be more likely to shy away from confronting (possibly) armed citizens, investigating certain suspicious activities, or engaging in effective crime-fighting actions due to the increased risk that guns pose to them. Moreover, Doucette et al. (2022) found that adoption of permitless carry laws between 2014 and 2020 caused a 13 percent average increase in officer-involved shootings and argued this effect may be because concealed carrying increases officers’ perceived risk of coming under fire in the line of duty. Increased

¹¹For a more thorough discussion of the theoretical mechanisms for RTC laws to affect crime, see Donohue, Aneja and Weber (2019).

¹²An established literature that has shown, on the margins, additional policing reduces crime rates (Levitt, 1997; Mello, 2019; Klick and Tabarrok, 2005).

killings by police can strain relations with the community in ways that impair the solving of crimes. If RTC laws degrade police effectiveness through this array of mechanisms, then the probability of facing legal sanctions decreases. In the context of our model, $p'(j) < p(j)$ and Term 3 will increase $EU' - EU$.¹³

The findings from our model indicate that if violent crime increases due to an RTC law, then the criminal response to RTC laws — whether through the facilitation effect or diminished police effectiveness — is unambiguously larger than the impact of the deterrence effect for the marginal potential criminal. Given our focus on illuminating empirically the mechanisms that generate the net adverse consequences of RTC laws, we do not model the effects of RTC laws on permit holders since our data does not directly address their behavior. We recognize, however, that increased carrying of guns by this group may also lead directly to more criminal acts, since a person legally carrying a concealed weapon would have more opportunity to reach for a gun in a moment of anger (e.g. when experiencing road rage (Goodman, 2022)), leading to more assaults and homicides.¹⁴ We also note that Becker’s framework only models a single individual, and we expect there to be heterogeneity in how perceptions of expected utility will change in response to RTC laws. In particular, we consider how career criminals may react differently than other individuals in our discussion section, below.

III. Data

The primary data used for our analysis comes from the 47 U.S. cities with a 2019 population estimate of 400,000 or more.¹⁵ We take agency-level monthly crime, clearance, and stolen property data cleaned by Jacob Kaplan from the FBI Uniform Crime Reporting (UCR) Return A files (Kaplan, 2021*b,c*) and Supplementary Homicide Reports (SHR) (Kaplan, 2021*d*) and aggregate to the city-year level using an agency-city crosswalk published by the Bureau of Justice Statistics (2018). We use SHR data for homicides because UCR data do not disaggregate firearm and

¹³It is also possible, however, that the probability of going to jail increases for a criminal who is shot or constrained by armed resistance. If this is the case, then an RTC law will have two effects on $p(j)$ that work in opposite directions, leaving the direction of term 3 ambiguous.

¹⁴Changes in permit holder behavior may also indirectly influence the utility for other individuals. First, permit holders may be emboldened to go to higher crime areas or carry more valuables, which can indirectly increase crime committed by others by increasing the availability of suitable targets and the size of payouts. Second, if the victim is carrying a gun on their person or in their vehicle, this can be taken as part of the robbery, increasing the expected income of the robbery (Ludwig and Cook, 2004). These mechanisms would increase the net expected income of robberies, particularly for firearm robberies, since robbers with firearms are less likely to shun a firearm-carrying victim. We do not address these mechanisms in our modeling, but they would act in the same direction as our theoretical and empirical findings, since the RTC-induced increase in income from committing a crime would rise (Y' increases), just as it does with the facilitation effect.

¹⁵While there are benefits to using more agencies to increase the power of analysis or to observe non-urban settings, there are also potential costs. Specifically, UCR agency crime data, whose biases in imputation procedures may already be attenuating our estimates (Boylan, 2019*a*), appears to have even greater reporting flaws for agencies serving smaller populations (Boylan, 2019*b*).

nonfirearm homicides.¹⁶

Our choice of control variables makes only one addition to the nine socioeconomic and demographic controls used in Kovandzic, Marvell and Vieraitis (2005) (hereinafter “KMV”), one of the most recent city-level studies of the effect of RTC laws. The KMV nine controls are the percentage of the population made up of female headed households, the percentage of people living alone, per capita income, percentage of people in poverty, and four demographic controls, all at the city level and obtained through Census and American Community Survey (ACS) data, as well as one-year lagged incarceration rates at the state-level from the Bureau of Justice Statistics. We add one-year lagged sworn officers per capita at the city level obtained from UCR police employment data (Kaplan, 2021a), due to the well-established relationship between police and crime.¹⁷

We apply a cleaning procedure that removed crime observations that were sharp discontinuities from the preceding and following year. We also removed all observations for a particular city-crime if that city-crime was missing more than 15 observations out of the 41 total city-crime observations from 1979 to 2019. This is because we suspect that the data with high degrees of missingness would lead to unreliable estimates for the years in which the particular crime data for that city does exist. This applies to all variables for Louisville; to homicide (and by extension, violent crime) in Jacksonville, Miami, and Omaha; to violent crime clearance in Chicago; and to the value of stolen guns in Chicago, Minneapolis, New York, Omaha, San Jose, and Washington DC. Further, if a city is missing data for any robbery-related outcome variable (total robberies, firearm robberies, or nonfirearm robberies) in a given year, we remove the data for all robbery related variables for that city-year to maximize interpretability of results within different robbery regressions. We do the same for homicide and aggravated assault. More details on our data cleaning procedure can be found in Online Appendix A.

Table 1 reports summary statistics from UCR crime, clearance, and stolen property data.¹⁸ Column 1 shows overall values for our entire sample, and columns 2-5 show these values for both 1979 and 2019 for never-adopters (the cities in our sample that were never covered by RTC laws), and for switchers (cities in our sample that adopted an RTC law between 1979 and 2019). A survey of the difference in the change in crime between 1979 and 2019 for never-adopting and switching

¹⁶While we recognize the SHR homicide data is less reliable than the CDC’s Vital Statistics data, only the former is available at the city level.

¹⁷In Online Appendix B, we show that that the removal of police as a control variable only increases the magnitude of our findings of the effect of RTC laws on crime.

¹⁸UCR defines aggravated assault as an “unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury. The UCR Program further specifies that this type of assault is usually accompanied by the use of a weapon or by other means likely to produce death or great bodily harm.” This is distinguished from simple assaults, which UCR defines as “[a]ssaults that do not involve the use of a firearm, knife or cutting instrument, or other dangerous weapon, and in which the victim did not sustain serious or aggravated injuries” (Federal Bureau of Investigation, 2020).

cities provides a preview of our main results reported in section IV, below. Violent crime decreases across both samples, but the never-adopting cities show much larger decreases, particularly for firearm violent crimes. Table 1 reveals that police effectiveness as measured by the clearance rates changed dramatically for the two sets of cities from 1979 to 2019. Over this period, clearance rates improved in never-adopting cities but worsened in cities adopting RTC laws. While the value of stolen guns per capita declined in both groups of cities, the decrease is larger in never-treated cities, both in absolute and percentage terms. These simple comparisons are consistent with our theory of the criminogenic effects of RTC laws, which is more fully explicated by the more complete statistical analysis below.

Table 1—: Summary Statistics

Variable	Overall	Never-Adopters 1979	Never-Adopters 2019	Switchers 1979	Switchers 2019
Violent Crime	11.28 (6.06)	15.39 (4.06)	6.04 (2.59)	8.87 (3.9)	7.81 (3.58)
Firearm Violent Crime	3.81 (2.57)	4.53 (1.04)	1.12 (1.24)	3.66 (1.9)	3.44 (2.23)
Robbery	5.19 (3.59)	9.25 (2.94)	2.13 (1.42)	4.84 (2.53)	2.27 (1)
Firearm Robbery	2.08 (1.56)	3.05 (0.79)	0.49 (0.75)	2.23 (1.34)	1.05 (0.61)
Homicide	15.98 (11.05)	23.06 (5.72)	6.61 (9.54)	23.95 (11.79)	13.31 (7.81)
Firearm Homicide	11.16 (8.51)	13.45 (3.87)	4.74 (8.79)	15.75 (8.51)	10.45 (6.67)
Aggravated Assault	6.05 (3.18)	5.9 (1.38)	3.84 (1.21)	3.9 (1.9)	5.27 (2.69)
Firearm Aggravated Assault	1.67 (1.21)	1.35 (0.45)	0.59 (0.45)	1.28 (0.72)	2.21 (1.65)
Violent Crime Clearance	0.41 (0.11)	0.32 (0.09)	0.51 (0.14)	0.46 (0.09)	0.36 (0.1)
Robbery Clearance	0.26 (0.09)	0.19 (0.06)	0.39 (0.12)	0.34 (0.1)	0.27 (0.1)
Homicide Clearance	0.66 (0.16)	0.61 (0.07)	0.78 (0.16)	0.75 (0.11)	0.61 (0.15)
Aggravated Assault Clearance	0.52 (0.13)	0.49 (0.1)	0.55 (0.15)	0.61 (0.11)	0.41 (0.11)
Stolen Gun Value	683.45 (466.89)	772.62 (518.16)	466.91 (371.86)	986.07 (538.45)	790.58 (473.8)

Note: All values are weighted by population, excluding values removed via cleaning procedure (see Online A). The units for violent crime, robbery, aggravated assaults are incidents per 1,000 population, while the units for homicide are incidents per 100,000 population. The unit for stolen guns is thousands of nominal dollars of firearms stolen per 1,000 population. Clearance rates are proportions between 0 and 1. Population-weighted standard deviations in parentheses. Owing to missing data on stolen guns for the large city of Los Angeles, the values reported in columns 2 and 4 for stolen guns are not 1979 values but 1986 values (the first year Los Angeles reported stolen guns data).

IV. Empirical Estimates

A. Methodology

Our two-way fixed effects model takes the following form:

$$(4) \quad y_{it} = \beta RTC_{it} + X_{it}\gamma + \alpha_t + \delta_i + \epsilon_{it}$$

where y_{it} represents some outcome variable in unit i at time t and X represents a set of covariates and a constant.¹⁹ The coefficient β reflects the average estimated treatment effect of adopting a RTC law on crime, and α and δ represent time and city fixed effects, respectively. For total crimes, firearm crimes, nonfirearm crimes, and clearance rates, we use 100 times the logged rate as the dependent variable; for stolen guns, the dependent variable is 100 times the logged monetary value of stolen guns per 1,000 people. We define violent crime as the sum of robbery, homicide, and aggravated assault; we exclude rape due to the change in definition of rape in UCR coding in 2013 and because rape data is not disaggregated by firearm and nonfirearm crime.²⁰ We cluster robust standard errors at the *state* level, the level at which “random assignment” occurs, since all RTC laws in our study are state-level changes. All regressions are weighted by time-varying city populations.

Our *RTC* treatment variable is coded as a 0 for each observation where concealed carrying is prohibited or a may-issue regime is in place and is coded as a 1 starting from when a “shall-issue” law goes into effect and remains a 1 even if a city moves to a permitless-carry system.²¹ Since RTC laws do not always go into effect at the start of the year, we code the first year as a fractional value that reflects the proportion of the year that the law is in place. Carrying forward our example of Texas from Section I, the RTC variable for Dallas is coded as a 0 in our sample from 1979

¹⁹While KMV use the natural logarithms of their control variables, we opt for unlogged covariates instead, as we expect that using unlogged control variables will intuitively be more likely to yield conditional parallel trends between our treated and nontreated units. Additionally, KMV include the lagged dependent variable as a control, but we opt for static panel model instead, since we do not expect that there are strong dynamic mechanisms causing current values of our dependent variables to be dependent on previous values, and the interpretation of the coefficients of the dynamic model is more difficult to conceptualize and compare to other results in the literature.

²⁰There are two other reasons why empirical researchers may want to consider rape separately from other categories of violent crime. First, the data quality for this crime are questionable. As the head of the Bureau of Justice Statistics stated to the National Research Council: “[rape and sexual assault] remain the darkest of the ‘dark figure’ of crime” (Kruttschnitt et al., 2014). Second, legal and social changes have encouraged women to report sexual violence, and police have become more likely to report assaults (Clay-Warner and Burt, 2005; Miczek, Reiss and Roth, 1993, 408-416). Thus, trends in the reporting of sexual violence could quite plausibly be changing temporally and geographically throughout our study window to a greater degree than for other types of crime. The study of the effect of RTC laws on this category of violence merits further research using more suitable data.

²¹The treatment variable is monotonic because no cities in our sample moved from a less-restrictive regime back to a more-restrictive one. This was the categorization scheme adopted in the original Lott and Mustard (1997) paper, maintained in the National Research Council et al. (2005) report on *Firearms and Violence*, and followed in Donohue, Aneja and Weber (2019).

until 1995, inclusive, during which time the Lone Star State prohibited concealed carry, and then switches to a 1 starting in 1996 because Texas became a shall-issue state on January 1, 1996. While different methods of capturing the time-varying effect of RTC are possible, we choose to estimate the average effect of RTC laws using a static model and then to provide event-study analyses that illustrate the change in the effect of RTC laws over time.

B. The Impact of RTC Laws on Violent Crime Rates and Its Underlying Mechanisms

VIOLENT CRIME. — Table 2 provides our first set of empirical results for our city-level analysis, using the specification in Equation (4). Our point estimates for violent crime, robbery, and aggravated assault indicate overall crime rises roughly 11 to 15 percent, with the firearm component increasing by roughly twice that level. While point estimates for nonfirearm crimes in these three categories are positive and sizable, the similarly large standard errors render inconclusive our findings on nonfirearm crimes. For violent crime and robbery, the roughly 30 percent increases in firearm crime are statistically significant at the 0.05 level and for firearm aggravated assault the 24.5 percent estimated increase is significant at the 0.01 level. Homicide is estimated to rise by 9 percent and firearm homicide by 13 percent, and nonfirearm homicide is estimated to drop by 3.4 percent, but these coefficients are not statistically significant at conventional levels (with p -values of 0.39, 0.29, and 0.70 respectively).²² Robberies is the violent crime subtype with the largest estimated impact of RTC laws, for both total crimes and firearm crimes. In our discussion, we suggest that the criminogenic mechanisms we proposed in our model may be particularly strong among individuals prone to commit robberies relative to individuals prone to commit other violent crimes.

Having confirmed the findings of the recent literature on the impact of RTC laws on violent crime rates, we now present empirical evidence concerning two possible criminogenic mechanisms: declines in police effectiveness and the facilitation effect.

DECLINES IN POLICE EFFECTIVENESS. — To empirically estimate the impact of RTC laws on police effectiveness, we use the same specification in Equation (4), using police clearance rates as our outcome variables. The first four columns of Table 3 provide these estimates controlling for the same set of covariates used in our crime regressions above. Across the four violent crime categories,

²²If one were just looking at the 3 homicide point estimates in Table 2, one might interpret them to mean that RTC laws have a very large impact in increasing firearm murders by over 12.5 percent but perhaps one-quarter of this increase comes from killers switching from other methods to firearms to complete their crimes. The -3.4 value for nonfirearm doesn't convey much information, though, since its standard error is more than 2.5 times as great as the point estimate.

Table 2—: Effect of Right to Carry on Crime, OLS Estimates

100 times log rate of ...						
	Violent Crime			Robbery		
	Total	Firearm	Nonfirearm	Total	Firearm	Nonfirearm
RTC	12.54 (7.48) p = 0.10	28.78 (11.94) p = 0.02	6.86 (6.71) p = 0.31	14.86 (10.02) p = 0.14	31.91 (14.62) p = 0.03	6.96 (7.88) p = 0.38
N	1,673	1,673	1,673	1,811	1,811	1,811
R ²	0.87	0.86	0.84	0.90	0.86	0.91
	Homicide			Aggravated Assault		
	Total	Firearm	Nonfirearm	Total	Firearm	Nonfirearm
RTC	8.52 (9.75) p = 0.39	12.62 (11.75) p = 0.29	-3.43 (8.86) p = 0.70	11.18 (7.00) p = 0.12	24.51 (9.30) p = 0.01	6.16 (7.87) p = 0.44
N	1,740	1,740	1,740	1,824	1,824	1,824
R ²	0.85	0.83	0.82	0.77	0.84	0.72

Note: City-level panel data estimates with city and year fixed effects, 1979-2019. Cluster-robust standard errors with clustering at the state level shown in parentheses. All models control for lagged sworn officers per capita as well as a set of nine unlogged KMV controls that are not shown to conserve space. Jacksonville, Louisville, Miami, and Omaha are dropped from homicide and violent crime regressions because of missing homicide data. Louisville is also dropped from robbery and aggravated assault regressions due to missing data. Violent crime, robberies, and aggravated assaults are measured as crimes per 1,000 population; homicides are measured per 100,000 population.

clearance rates fall by roughly 7.5 to 15 percent due to RTC laws.²³ The statistically significant 13.0 percent drop ($p = 0.03$) in the clearance rate of all violent crimes is striking. Given the generally low rate at which violent crimes are cleared, these RTC-induced reductions in the ability of police to detect and sanction violent criminals are noteworthy and troubling.

One reason that clearance rates might fall in the wake of RTC adoption is that the crime increases resulting from the new regime burdens the police, thereby impairing their ability to clear crimes at the same rate. For example, the police only have the ability to solve 40 out of 100 crimes, if crime rises by 20 percent and they still can only solve 40 crimes, the clearance rate would fall from 40 percent to 33 percent (40 out of 120). To determine if this factor alone explains some or all of the RTC-induced drop in clearance rates, columns 5-8 in Table 3 re-estimate our clearance rate regressions controlling for violent crime rates. The resulting estimates show that the estimated decline in the clearance rate ranges from 7 to 14 percent.²⁴ This suggests that the RTC-triggered

²³Note that while our preceding analysis used SHR homicide data, the variable for violent crime clearance rate in this section is based on UCR homicide data, since those are the only homicide clearance data available.

²⁴Note that the regression estimate on the column (5) controlling for violent crime is likely biased downward by the presence

decline in police effectiveness is primarily caused by factors other than the overall increase in violent crime that RTC laws stimulate, such as the burdens on police time caused by greater gun carrying, police hesitation to engage with a more heavily armed civilian population, or weakened police-community relations.

Table 3—: Effect of Right to Carry on Clearance Rates

	100 times log clearance rate for ...							
	Violent Crime	Robbery	Homicide	Aggravated Assault	Violent Crime	Robbery	Homicide	Aggravated Assault
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RTC Law	−12.96 (5.89) p = 0.03	−14.90 (6.63) p = 0.03	−7.48 (4.39) p = 0.09	−14.29 (7.54) p = 0.06	−11.79 (5.98) p = 0.05	−10.41 (6.51) p = 0.12	−6.79 (4.22) p = 0.11	−13.64 (7.52) p = 0.07
Log Violent Crime Rate					−6.91 (6.02) p = 0.26	−31.47 (10.08) p = 0.002	−4.48 (5.03) p = 0.38	−4.65 (8.63) p = 0.60
Observations	1,484	1,667	1,550	1,667	1,484	1,667	1,550	1,667
R ²	0.55	0.58	0.40	0.44	0.55	0.61	0.40	0.44
Adjusted R ²	0.52	0.56	0.36	0.41	0.52	0.59	0.36	0.41

Note: City-level panel data estimates with city and year fixed effects, 1979-2019. Cluster-robust standard errors with clustering at the state level shown in parentheses. All models control for lagged sworn officers per capita as well as a set of nine unlogged KMV controls but are not shown to conserve space. Jacksonville, Miami, and Omaha are dropped from homicide clearance and violent crime clearance regressions because of missing homicide data. While Chicago is included in robbery clearance, homicide clearance, and aggravated assault clearance regressions, it is dropped from violent crime clearance regressions because when these categories are combined, the amount of missing data is excessive.

INCREASES IN GUN THEFT. — We also estimate the impact of the facilitation effect by examining the effect of RTC on reported gun theft. In Table 4, we regress 100 times the logged monetary value of stolen guns per 1,000 persons on our RTC dummy, controlling for city and year fixed effects, and our standard set of covariates. We find that RTC laws are associated with a sizeable 35 percent increase in stolen gun value per capita ($p = 0.06$). Assuming a constant average value per stolen gun, then for roughly every three guns that were being stolen before RTC, four are being stolen after. And if the magnitude of our finding on the effect of RTC laws on stolen guns in major US cities is roughly the same as the effect of RTC laws on stolen guns across the US, then we would predict that over 100,000 guns were stolen in 2015 due to RTC laws,²⁵ quite similar to the estimate

of ratio bias. This occurs because overall violent crime is in the denominator of the clearance rate dependent variable and in the numerator of the independent variable, which will bias the estimate downward because violent crime is measured with error. The same rationale also applies, albeit to a lesser extent, to the regressions in columns (6)-(8), since violent crime will also be correlated with the component crimes of overall violent crime.

²⁵Hemenway, Azrael and Miller (2017) estimates that 380,000 guns were stolen in the US in 2015, and Bureau of Alcohol, Tobacco, Firearms and Explosives (2015) indicates that over 85 percent of National Firearms Act registered weapons were from RTC states. If we assume that this suggests that 85 percent of gun thefts occurred in RTC states and that RTC laws increased gun thefts by 35 percent, then there were about 113,000 RTC-induced gun thefts in 2015. Given the upward trend in gun thefts that Azrael and Miller observed over their data period, as well as the increased number of RTC and permitless jurisdictions with the attendant greater carrying of guns outside the home, the total number of gun thefts in the country and those induced by more permissive gun carrying are likely higher today than these figures based on 2015 data.

provided by (Donohue, Aneja and Weber, 2019) based on survey data showing higher rates of gun thefts for those who take the gun outside the home.²⁶

Table 4—: Effect of Right to Carry on Gun Theft

100 times log dollar value per 1,000 persons of ...	
Stolen Guns	
RTC Law	34.77 (18.36) p = 0.06
Observations	1,538
R ²	0.78
Adjusted R ²	0.77

Note: City-level panel data estimates with city and year fixed effects, 1979-2019. Cluster-robust standard errors with clustering at the state level shown in parentheses. All models control for lagged sworn officers per capita as well as a set of nine unlogged KMV controls but are not shown to conserve space. Chicago, Louisville, Minneapolis, New York, Omaha, San Jose, and Washington DC are dropped because of excessive missing data.

C. Addressing Potential Concerns

We supplement our main empirical analyses with an event-study analysis as well as Goodman-Bacon (2021) decompositions of our regressions. The event-study analysis gives us a framework for investigating the validity of the conditional parallel trends assumption in our empirical context as well as the evolution of the impact in RTC laws over time. Goodman-Bacon (2021) decompositions provide insight into the robustness of our regressions to heterogeneous treatment effects. We also perform covariate balance tests using the weights obtained from the Goodman-Bacon (2021) decomposition to assess how well the regression counterfactual matches the treatment group on observable characteristics.

EVENT-STUDY ANALYSIS. — We employ an event-study design to allow us to inspect the stability of our pre-passage time trends and examine how the impact of RTC law evolves over time. The findings of a DiD OLS design such as those presented in Table 2 depend on the following assumption: conditional on covariates, if treated units never adopted the treatment, their outcomes would have evolved in parallel to the untreated units. In our empirical context, this assumption might not hold if, for instance, selected states were experiencing a secular increase in violent crime rates in their

²⁶The specific estimate in (Donohue, Aneja and Weber, 2019) was that of the roughly 400,000 guns stolen each year, the RTC-induced increase in thefts had increased that number from 300,000 to 400,000.

major cities and adopted RTC laws as a response. As stated previously in Section I, we argue that the adoption of RTC laws is unrelated to pre-existing crime trends.

While this parallel trends assumption cannot be tested directly because we cannot observe the counterfactual city, we can examine whether conditional parallel trends held in the period prior to RTC adoption. To do this, we ran regressions including the values on yearly dummy variables for each of the 10 years prior to RTC adoption to 10 years after RTC adoption as well as catch-all variables for 11 or more years before and 11 or more years after RTC adoption, to measure the dynamic effect of the policy change in the cities that switched RTC laws during our study window. The dummy for one year prior is omitted, so all coefficients are normalized to the value in the year prior to RTC adoption. Equation 5 shows the model used for our analyses: let *AdoptYear* be the rounded year of RTC implementation for the cities that came under a RTC regime during our 1979-2019 data period, and equal to zero for all other cities. That is, with some shorthand notation for the “11-or-more” variables, we computed the least squares fit for:

$$(5) \quad y_{it} = \sum_{k \in \{-11+, -10, \dots, 10, 11+\} \setminus \{-1\}} \beta_k \mathbf{1}[t = \text{AdoptYear}_i + k] + X_{it}\gamma + \alpha_t + \delta_i + \epsilon_{it}$$

We then plotted the point estimates and confidence intervals of each of the yearly dummies β_k . Figure 3 presents these event-study estimates for the firearm violent crime and firearm robbery rate. We see that in both cases, the trend is flat in pre-passage years and rises sharply following RTC adoption. We find a similar pattern for overall robberies and overall violent crime rates, as well as overall aggravated assault and firearm aggravated assault rates. Our plots suggest that the adoption of RTC laws leads to sharp increases in these crime rates in the first three years, followed by a far more slowly rising crime rate thereafter. A similar pattern is also seen for both clearance rates and stolen guns. The finding that in RTC-adopting cities crime clearance starts dropping and the value of stolen guns starts increasing at the same time crimes start rising lends support to our hypothesis that declines in police effectiveness and the facilitation effect are key mechanisms by which RTC laws generate increased violent crime.

A standard test for assessing the parallel trends assumption is to establish that one cannot reject the assumption that the yearly dummies from two up to five years prior to adoption are jointly different from zero. With the exception of nonfirearm aggravated assault, we find non-significance at the ten percent level for all F-tests on our crime rate, clearance rate, and stolen guns outcome variable regressions. Event-study plots and F-tests for all outcome variables are provided in Online

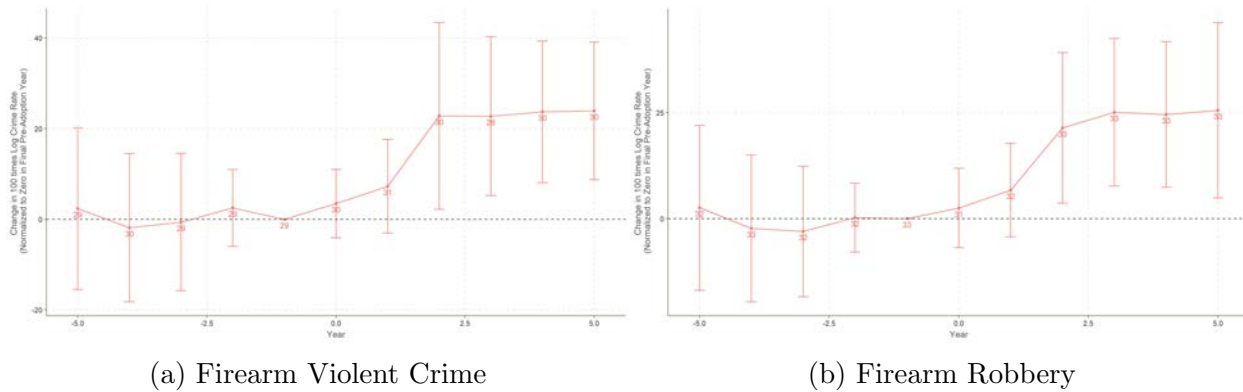


Figure 3. : Firearm Crimes Rise Sharply at the Point of Adoption of RTC Laws

Note: 95 percent confidence intervals with cluster-robust standard errors displayed. The number of switcher cities contributing to each coefficient are indicated on the chart.
Source: Author-cleaned data set.

Appendix D.

Beyond our F-tests and visual inspections of the event-study analyses, we also note that for all our statistically significant outcome variables, any deviations from parallel pretrends are small relative to the treatment effect. Manski and Pepper (2018) used partial identification analysis to study the effect of RTC laws on crime in three states (albeit using an incorrect date for RTC passage for the state of Virginia). As a quantitative analysis in the spirit of Manski and Pepper (2018), we consider the magnitude of our estimated treatment effects relative to any prepassage deviation from parallel pretrends. For example, consider how our point estimate that RTC laws increase firearm violent crime by 28.96 percent compares to the largest deviation from zero in any of the five placebo years prior to RTC passage, $\hat{\theta} = \max\{|\hat{\beta}_{-5}|, \dots, |\hat{\beta}_{-2}|\} = 2.50$. The estimated treatment effect is $28.96/2.50 = 11.58$ times as large as this $\hat{\theta}$, meaning that the magnitude of the deviation from parallel trends in the *post*-treatment period that would be required to reverse the sign of our average treatment effect is more than 11 times as large as any deviation observed in the five years prior to passage.

HETEROGENEOUS TREATMENT EFFECTS. — Goodman-Bacon (2021) illustrates how heterogeneous treatment effects and trends could undermine the robustness of the staggered DiD OLS estimator of the average treatment effect on the treated (ATT), and provides a method for decomposing the estimates to assess their validity. Goodman-Bacon demonstrates that the overall ATT estimate is a weighted average of many “2x2” DiD comparisons divided into the following five categories:

- 1) Timing Group vs Timing Group: This is the combination of 2 different cases:
 - a) An earlier treated group as the treatment group with a later treated group as a control;
or
 - b) A later treated group as the treatment group with an earlier treated group as the control
- 2) A treated group as the treatment group with the always-treated group as the control;
- 3) A treated group as the treatment group with the never-treated group as the control;
- 4) The always-treated group as the treatment group with the never-treated group as the control;
- 5) Within Timing Groups: using differences in covariates as variation, this category compares two units that implemented an RTC law at the same time to each other.

Only comparison 3 (treated group vs. never-treated group) remains unbiased if treatment effects change over time. How heavily weighted a 2x2 DiD comparison will be is related to the population of the control and treatment units in that comparison, as well as the timing of the change in the RTC law in the treatment group (since groups that change their treatment indicator in the middle of the time frame of a study tend to be more heavily weighted than those who changed their treatment indicator at the beginning or end of the study period). All weights vary between 0 and 1 and sum to 1. Note that due to limitations in the Stata program “BACONDECOMP” (Goodman-Bacon, Goldring and Nichols, 2019), we are unable to use population-varying weights for the Goodman-Bacon decomposition, so we instead used fixed weights within each city based on their average population throughout the study period. Additionally, Goodman-Bacon decompositions currently do not consider partial treatment values, so we round all of our treatment values meaning that all states that adopted an RTC law in a given (rounded) year are considered to be part of the same treatment group. Lastly, the procedure does not support unbalanced data, so we used a multiple imputation procedure by Honaker, King and Blackwell (2011) to fill in missing data. In Online Appendix C, we explain the imputation procedure and replicate the main findings using our balanced dataset of imputed values and show that the results do not change substantially from those of our complete case analysis.

The Goodman-Bacon decomposition results are supportive of our conclusions. The calculated ATTs and relative weights of each type of comparison (the category “Timing Groups” combines both comparisons 1 and 2, which are currently not able to be disaggregated with the current Stata program when including control variables) for the regressions on firearm violent crime and firearm

robbery are presented in Table 5. In Online Appendix E, we provide equivalent tables for all crime outcome variables, and we also present decomposition plots showing the weight and ATT of each individual comparison. As illustrated in Table 5, the never vs. timing comparisons, which are not vulnerable to bias from heterogeneous treatment effects over time, are the most heavily weighted type of comparison. In every model the single most heavily weighted comparison of any two groups is the 1996 treatment group, consisting of 12 cities, to the 11 never-adopting cities.²⁷ Across all regressions, the never-treated vs. timing comparisons consistently have higher estimated treatment effects greater than or roughly equivalent to the overall group of 2x2 comparisons, suggesting that the removal of “unclean” comparisons would increase our aggregate ATTs.

Table 5—: Goodman-Bacon (2021) Decomposition Results

	Firearm Violent Crime		Firearm Robbery	
	Beta	Weight	Beta	Weight
Timing Groups	-11.27	0.40	-11.38	0.42
Always vs. Timing	7.58	0.02	14.45	0.02
Never vs. Timing	39.93	0.43	44.90	0.42
Always vs. Never	6.16	0.001	20.80	0.001
Within	-52.71	0.15	-34.95	0.14

Note: OLS decomposition weights for the regression on firearm violent crimes and firearm robberies from Table 2, with imputed data, rounded treatment values, and fixed 2019 population weighting. Missing data imputed using imputation procedure described in Appendix A.2.4.

The Goodman-Bacon decomposition allows us to identify the weight of each 2x2 comparison in the regression. Using this information, we can conduct weighted balance tests of the covariates used in our regressions. Following the reweighted balance testing procedure proposed in Goodman-Bacon (2019), we determine the “balance weight” for each timing group (e.g all cities that passed RTC laws in 1996 constitute a single timing group) based on the relative weight of the comparisons in which it is used as a treatment group versus the comparisons in which it is used as a control group. We then run a cross-sectional regression of each covariate on a dummy variable representing whether a timing group is weighted more as a treatment group than it is a control group in the overall regression. These regressions are weighted by each timing group’s “balance weight.” The reweighted balance test provides a more accurate assessment of the similarity (with respect to covariates) of the treated vs. counterfactual comparison in the regression than a traditional balance test.

Table 6 indicates reasonable covariate balance between our treated and counterfactual groups.

²⁷The 1996 treatment group consists of: Austin, Charlotte, Dallas, El Paso, Fort Worth, Houston, Las Vegas, Oklahoma City, Philadelphia, Raleigh, San Antonio, and Tulsa. The never-treated cities are: Baltimore, Boston, Fresno, Long Beach, Los Angeles, New York, Oakland, Sacramento, San Diego, San Francisco, and San Jose.

Table 6—: Goodman-Bacon (2021) Reweighted Balance Test, Firearm Violent Crime

	Percent 18 to 24	Percent 25 to 44	Percent Black	Percent Hispanic	Percent Female Headed Households
Treatment Group	0.16 (0.71) p = 0.83	−0.99 (0.65) p = 0.13	−0.19 (7.00) p = 0.98	0.89 (9.01) p = 0.93	−0.76 (1.51) p = 0.62
N	1,763	1,763	1,763	1,763	1,763
	Percent Living Alone	Percent in Poverty	Per Capita Income	Lagged Incarceration	Lagged Officers
Treatment Group	−0.68 (1.13) p = 0.55	−1.09 (1.45) p = 0.46	−444.13 (167.08) p = 0.01	119.89 (45.78) p = 0.01	−0.01 (0.58) p = 0.99
N	1,763	1,763	1,763	1,763	1,763

Note: To create balance weights, we sum the decomposition weight for all 2x2 comparisons where a group k is the treatment group and subtract the sum of the decomposition weights for all 2x2 comparisons where group k is the control group. We then create a dummy that equals one for all groups k for whom the balance weight is positive. The coefficients in the table represent the results of a cross-sectional regression of each covariate on that dummy, weighting by the balance weight. To match the analysis on firearm violent crime, Louisville, Omaha, Miami, and Jacksonville are dropped from the regression.

Most covariates show only small differences between the treated and counterfactual groups that are not statistically significant at a meaningful level. While the difference in per capita income between the treated and counterfactual groups that is significant at the 5 percent level, the difference of \$444 is marginal compared to the average per capita income in our sample of \$3921 (both values in 1967 dollars). As noted in Donohue, Aneja and Weber (2019), RTC states had larger increases in incarceration than other states following RTC adoption, and we observe that treatment observations have higher incarceration levels in our panel. If we restrict the comparison to only the years prior to RTC, however, we find no statistically significant difference between ever-treated and never-treated cities (see Online Appendix F for details). Thus, we conclude that generally the regression counterfactual is reasonably similar to the treatment group on observable covariates.

V. Discussion

In this section, we revisit our theoretical model and discuss the implications of our empirical findings for how we understand the effect of RTC on criminal behavior and policing. We then explain why the greater proportion of robberies committed by “career criminals” may explain the more sizable effects for RTC on robbery compared to homicide and aggravated assault. Finally, we compare our empirical results to other findings from the recent literature.

A. Deterrence is Outweighed by Criminogenic Effects of RTC Laws

One of the major theoretical and empirical shortcomings of the policy discourse concerning the impact of laws promoting gun carrying outside the home is the implicit assumption that there are only two sets of actors relevant to the inquiry: criminals who will tend to be deterred by RTC laws and permit holders who may use their guns in a criminal fashion, either in a moment of anger or provocation or in some opportunistic act. This paper widens the lens with a theoretical model that shows that much of the prior literature has overlooked the numerous unintended consequences of RTC laws, which uniformly tend to elevate crime. Specifically, the prior literature has largely ignored the ways in which police and criminals respond to RTC laws in ways that increase crime and its social costs.

Recall that our theoretical model suggests that the impact of RTC laws on crime can be decomposed into three key effects: the facilitation effect (increased ease of obtaining and using guns) and declining police effectiveness both work to increase crime, and the deterrence effect (increased threat of retaliation from their victims, thereby increasing the perpetrators risk of injury, death, or arrest) pushes in the opposite direction.²⁸ The literature appears to be converging on the finding that the net effect of RTC laws is to significantly elevate violent crime (Cook and Donohue, 2017; Donohue, 2022), and this paper adds to this literature by once again illustrating the harmful net effect of RTC laws both with an updated state-level panel data analysis and also using a new data set of major U.S. cities. The most important new contribution of this work goes further to establish empirically how changes in the behavior of criminals and police following the adoption of RTC laws stimulate crime. While permit holders will also be influenced by the passage of RTC laws in ways both good and ill, this paper shows that the criminogenic effects of RTC laws broadly stimulate firearm violent crimes.

Our finding that the net effect of RTC laws is crime-inducing implies that the combination of the criminogenic influences of RTC laws outweigh any associated deterrence effect associated with RTC adoption, though we still have not empirically tested for the direct presence of any deterrent effect.²⁹ Recent work on legal expansions on defensive use of force have also found that any beneficial

²⁸As noted in Section II, the increased threat of facing an armed victim may cause some criminals to respond not by foregoing crime, but rather by carrying out offenses with firearms whereas they previously would have used another weapon or gone unarmed.

²⁹There is anecdotal evidence of some thwarting, injuring, and killing of criminals by permit holders as well as anecdotal evidence of permit holders inadvertently killing noncriminals or trying to portray their own criminal assaults as self-defense gun use. Prior empirical research has linked observable victim precaution measures, such as private security (Meehan and Benson, 2017), security alarm systems (Zimmerman, 2014), and neighborhood public safety organizations (Cook and MacDonald, 2011), to lower rates of robbery. Ayres and Levitt (1998) analyze the staggered rollout of Lojack across US geographic markets and find that the introduction of Lojack decreased auto-theft rates, which suggests that the deterrence effect of unobservable victim

deterrence they afford has been outweighed by their criminogenic effects. Specifically, Cheng and Hoekstra (2013) and McClellan and Tekin (2017) find that the adoption of stand-your-ground laws on balance escalates violence and has led to statistically significant increases in homicides. Like these two studies, we are unable to directly measure the deterrence effect of increased firearm carrying on crime, but we show that such an effect, if it exists, is small compared to the criminogenic effects of RTC laws. This point is driven home most dramatically by the fact that we find that RTC laws induced large *increases* in robberies since this is the crime most often committed outside the home with the largest potential for RTC-induced deterrence (Cook, Moore and Braga, 2004).³⁰

HOW LARGE IS THE POLICING EFFECT?. — Our results showed that RTC laws decreased the police clearance rates across many violent crime categories. Our finding that the drop in clearance rates does not change substantially when controlling for the rate of violent crime suggests that the decrease in clearance rates is driven by causes other than police being overwhelmed with the need to address increased violent crime. Possible other factors could include the taxing of police resources to deal with the RTC-induced increases in gun thefts, accidental gun discharges and shootings, processing of gun permits, and the array of factors that flow from police interactions with a more heavily armed public. Specifically, police effectiveness may be undermined because officers may be more hesitant to engage with a more heavily armed civilian population (Donohue, Aneja and Weber, 2019).³¹ In addition to this police “pull-back” effect, there is little doubt that police react more aggressively because of their fears about being shot, and the deaths and beatings at police hands that emanate from this fear can degrade police-community relations in ways that further dampens police effectiveness at solving crime (as the community pulls back from assisting the police).

Table 3 revealed that RTC laws cause the probability of arrest for violent crime to fall by 12.96 percent, which would translate to about a 5.4 percentage point reduction relative to the population-weighted average clearance rate of 42 percent in adopting cities the calendar year immediately

protection can have positive externalities even for unprotected victims. It will take further work to establish if there is any similar protective effect from concealed carry, and also to ascertain whether and to what extent criminals respond with greater violence to all potential victims because they cannot observe those who are carrying weapons.

³⁰Cook, Moore and Braga (2004) also notes that Lott (2000), which supersedes the work from Lott and Mustard (1997), provides little support for RTC laws reducing robbery, while his reported results for homicide are generally negative and statistically significant. Cook, Moore and Braga (2004) suggests that such findings are inconsistent with the predicted deterrence effect of RTC laws.

³¹This effect is distinct from the de-policing hypothesis in response to “viral” incidents of deadly force on police effectiveness studied in Devi and Fryer Jr (2020) and other literature investigating the potential “Ferguson effect,” as discussed in Donohue (2017). We note that work in this area is still preliminary, and police fear for their personal safety due to the potential increase in confrontations with armed civilians is distinct from police concerns about the consequences of the increased salience of police activity.

preceding RTC adoption.³² Previous empirical estimates place the elasticity of violent crime with respect to arrest rates in the neighborhood of -0.225 to -0.9, and between -0.26 and -0.5 for the elasticity of robbery with respect to arrests (see Bun et al. (2020), pp. 2308-2310, for a summary of estimates). If the true elasticity falls within the range given by the literature, then our empirical estimates would imply that impaired police effectiveness due to RTC laws could be responsible for an increase in violent crime of roughly 3 to 12 percent. This seems within the right order of magnitude compared to our estimate of the aggregate effect of RTC laws (which includes the facilitation and deterrence effects) driving a 13 percent increase in violent crime rates.

HOW LARGE IS THE FACILITATION EFFECT?. — Our empirical analysis also suggests that RTC laws increase the total value of stolen guns by around 35 percent, providing the first causal estimate of the effect of RTC laws on gun thefts. Compared to the literature on the elasticity of crime with respect to arrests, fewer studies have estimated the relationship between crime and stolen guns. Khalil (2017) estimates that the elasticity of firearm assaults with respect to illegal firearm flows within the last year is 0.15, which would suggest that the 35 percent increase in stolen guns due to RTC laws would lead to a 5 percent increase in firearm assaults.³³ Khalil's estimated harm from gun theft may also be understated since it does not consider how illegal gun flows may cross borders, creating negative spillover effects. Studies of firearms recovered in Chicago between 2009 and 2013 (Cook et al., 2014) and Boston between 1991 and 1995 (Braga, 2017) suggest that around 60 to 70 percent of guns recovered come from out of state, and Knight (2013) suggests that guns may flow from states with weaker gun laws to those with stronger gun laws. The large externalities associated with increased gun theft in a particular jurisdiction suggest that our estimates understate the harmful impact of RTC laws on crime and clearance rates.

CRIMINOGENIC MECHANISMS AND THE PROPORTION OF VIOLENT CRIMES COMMITTED WITH A FIREARM.

— For both robberies and overall violent crimes, our results showed a strong and significant increase in firearm crime, positive but insignificant point estimates on nonfirearm crimes, and marginally

³²Similarly, the estimate that robbery clearances fall by 14.90 percent due to RTC (Table 3) translates to a 4.0 percentage point decline in the probability of arrest relative to the weighted average clearance rate of 27 percent the year prior to RTC adoption in our sample.

³³While the Kahlil study is impressive in many respects, Kahlil acknowledges the challenges for his estimation strategy without plausibly exogenous variation of gun theft. Nonetheless, this estimate may be within the right order of magnitude. Prisoner surveys suggest that less than three percent of incarcerated individuals who used a gun to commit a felony stole the gun themselves but about a quarter to a third of them obtained guns on the black market (and an unknown proportion obtained the gun from a friend or relative who obtained the weapon illegally). Assaults are by far the most common crime committed with firearms, so an estimated elasticity of 0.15 of firearm assaults with respect to illegal gun flows within the last year does not seem unreasonable.

significant positive effects for overall crime. The difference in magnitude of findings on firearm and nonfirearm crimes indicate that in addition to increasing total rates of violent crimes, RTC laws also shift the composition of crimes towards more frequent criminal firearm usage. This pattern is consistent with our hypothesis that both the facilitation effect, which would increase the utility of firearm crimes, and declines in police effectiveness, which would increase both the utility of firearm and nonfirearm crimes, contribute to increases in crime linked to RTC laws. It is also consistent with the possibility that criminals arm themselves proactively due to the perceived increased threat of meeting armed resistance. Aside from providing further evidence bolstering our hypothesis, the shifting composition of violent crimes towards increased firearm usage is striking in and of itself due to its societal implications. Firearm robberies are far more likely to result in murder and serious injury than nonfirearm robberies, which increases the social burden from RTC laws (Cook, 1987). Moreover, firearm robberies are more lucrative, and therefore more costly to society, than nonfirearm robberies.

B. RTC's Heterogeneous Effects by Crime Type

The statistically significant estimates that RTC laws increase overall firearm violent crime as well as the component crimes of firearm robbery and firearm aggravated assault by remarkably large amounts with an attendant finding of no sign of any benefit from RTC laws represent a remarkable indictment of permissive gun carrying laws. Perhaps the most noteworthy and novel result is the finding that RTC laws increase firearm robbery by a striking 32 percent. Our estimates of the varying declines in police effectiveness across different crime types, as well as Khalil (2017)'s findings on the elasticity of stolen guns relative to different crime types, suggest that it is reasonable to expect heterogeneous effects of RTC laws on different types of violent crime.

One key distinction between robberies and other components of violent crime that may be driving heterogeneous treatment effects is that a significantly larger share of robberies are committed by "career criminals."³⁴ These criminals differ from other individuals in their risk assessment due to their experience in criminal settings (Anwar and Loughran, 2011). We reason that most will respond to RTC laws not by ending their criminal careers but rather by adjusting their behavior to continue to effectuate their criminal designs by using guns themselves and more aggressively confronting their victims to foreclose the prospect of armed resistance. They are aided in these

³⁴While the share of each type of violent crime that is committed by career criminals is difficult to determine, a proxy for this may be the share of each type of crime that is committed by strangers. National crime victimization data suggests that the proportion of robberies committed by strangers is substantially higher than the proportion of aggravated assaults or homicides committed by strangers (Harrell, 2012).

efforts by the law-abiding citizens who carry guns outside the home, thereby leading to their theft on the order of about 100,000 stolen guns per year and by the cascade of influences operating on police that ultimately result in lower police effectiveness, as shown in our documentation of substantial drops in the clearance rates of violent crime.

Since an influx of stolen guns and diminished police effectiveness are both factors that can lead to the success and profitability of criminal misconduct, we would expect that the criminogenic effects of RTC laws are likely to be stronger among career criminals than other potential criminals. The illicit facilitation effect mechanism through increased gun theft will disproportionately aid career criminals, and we suspect for three reasons that they will perceive the increased risk of victim retaliation to be smaller and the decreased risk of legal sanctions to be larger than other individuals will. First, because of their past experiences in crime, career criminals are likely to better assess the increased risk posed to them by an armed victim, and are thus less likely to inflate the increased risk due to risk averse preferences (Schulz, 2014). Second, their past experiences, social networks, and active interest in observing law enforcement, will make them more likely to perceive the RTC-induced declines in police effectiveness. Third, career criminals may be more selective about their victims and may simply choose to commit crimes against victims they predict are less likely to be armed or engineer their attacks in ways that thwart any defensive response.

C. Situating Our Findings in Existing Literature

By applying the methodology from Goodman-Bacon (2021), we join the emerging literature applying new econometric techniques that provides evidence of the link between RTC laws and violent crime. Colmer and Doleac (2021) avoid many of the challenges we describe in the robustness section of our paper by instead studying how RTC laws affect the homicide-temperature relationship, finding that lenient gun laws lead to substantially greater increases in homicide as temperatures rise. McElroy and Wang (2017) employ a “seemingly inextricable dynamic differences (SIDiD) estimator” using a proxy for age-specific violent crime rates to study entry and exit behaviors of violent criminal cohorts and find that RTC laws led to a substantial increase in violent crime.

Comparison of our current city-level analysis of 1979-2019 data to the state-level panel data analysis for 1979-2014 in Donohue, Aneja and Weber (2019) may also be a useful benchmark, as both studies use a parsimonious set of socioeconomic and demographic covariates and are similar in many of their other specifications. Our present study has a higher estimate of the effect of violent crime, although the two estimates are not statistically distinct when compared with a two-sample

z-test (Clogg, Petkova and Haritou, 1995). If it is the case that RTC laws cause a larger increase in violent crime in our present study sample than in the study sample of Donohue, Aneja and Weber (2019) and in the updated state-level estimates we provide in this paper, one reasonable interpretation is that the harmful effect of RTC laws on violent crimes is greater in large urban areas than in other parts of the country. This may be due to the fact that there are more guns to steal in cities or that police effectiveness degrades more in cities, where most violent crimes occur.

VI. Conclusion

Using a novel data set on crime in the most highly populated US cities, we showed that RTC laws cause an increase in firearm violent crimes, robberies and aggravated assaults, and provide suggestive evidence of increases in overall violent crimes, robberies and aggravated assaults as well. We emphasize the importance of rigorous robustness checks of the various assumptions made in an empirical model. To that end, we use both context-dependent qualitative and quantitative demonstrations of the robustness of our population-weighted least squares regression to the possibility of non-parallel pretrends and bias due to heterogeneous treatment effects.

We then provide important new information on two mechanisms that underlie these increases in crime following the adoption of RTC laws. The increasing firearm violence is facilitated by a massive 35 percent increase in gun theft ($p = 0.06$), with further crime stimulus flowing from diminished police effectiveness, as reflected in a 13 percent decline in violent crime clearance rates. Taking the midpoint of the relevant elasticities discussed above, these two factors would generate an 8.7 percent increase in violent crime, when the total increase in violent crime from RTC laws is estimated to be 13 percent.³⁵ On this accounting, roughly two-thirds of the increase in violent crime resulting from RTC laws is caused by impaired policing and increased gun theft. It is plausible that two other factors whose individual effects we have not been able to estimate in this study contribute to this increase in firearm violence: 1) criminals who previously committed crime without carrying guns decided to arm themselves in response to the increased potential of armed resistance and 2) some permit holders responded to stressful situations by engaging in criminal violence with their newly carried weapons. At the same time, any benefits from deterrence or thwarting/incapacitating

³⁵We noted that impaired police effectiveness due to RTC laws could increase violent crime by roughly 3 to 12 percent, so the above calculation takes the midpoint value of 7.5 percent. Using Khalil (2017)'s measure of determining elasticities of each component of violent crime with respect to stolen guns, we estimate that overall firearm violent crime has an elasticity of about 0.10 with respect to stolen guns, based on the relative proportion of each sub-type of firearm violent crime. Given that firearms were used in about 34 percent of violent crimes in our sample, a reasonable estimate is that the elasticity of violent crime with respect to gun theft is in the neighborhood of 0.034. This means that a 35 percent increase in stolen guns would lead to an additional increase in violent crime of 1.2 percent. The combined increase in violent crime from these (admittedly imprecise) estimates would be $7.5 + 1.2 = 8.7$ percent.

criminals from increased gun carrying under RTC laws would dampen crime. All we can conclude at this time is that the combined effect of these unobserved factors seems to explain only half as much of the violent crime increase as the mechanisms we have measured.³⁶

These findings are illuminating for both policymakers and researchers considering the effect of different types of gun laws on criminal behavior. Our study investigates the criminogenic effects associated with increased gun carrying. The same mechanisms we identify in our paper with respect to increased gun carrying are relevant in other policy contexts as legal changes that promote or decrease gun theft will presumably have predictable repercussions for criminal activity. Similarly, the end of the federal assault weapons ban and the attendant federal ban on high-capacity magazine ban or eliminating gunfree zones might well be associated with declines in police effectiveness. Certainly, the experience in the Parkland High School mass shooting in 2018 and the Uvalde mass shooting of 2022 were examples of police reluctance to confront a teenage killer armed with an AR-15. A key contribution of our article is to advance our understanding of the mechanisms governing criminal and police responses to gun laws, thereby clarifying how these laws may affect crime and public safety.

Our findings suggest a number of avenues for future research. First, while our study contributes to the literature finding that RTC laws elevate crime on balance, more work needs to be done to tease out the individual contributions of the three factors whose combined effect (but not necessarily their individual effects) is likely to be small in comparison to the criminogenic effects we have identified herein. For example, it will be interesting to nail down whether there is any detectable benefit associated with these laws and if the burdens of the law that we have clearly identified fall equally on permit holders and others. Second, given the right data, it may be possible to clarify the reasons for RTC-induced declines in police effectiveness by measuring whether RTC laws reduce police-civilian interactions, effectively pose a tax on police time, degrade police-community relations, or diminish risky but effective crime-suppressing police interventions or operations. Third, more granular data on the black market for stolen guns and the flow of illegal firearms could clarify the extent to which firearms stolen under an RTC regime increase crime by converting thieves into violent criminals or simply increasing the weaponry available to violent criminals. Finally, Goodman-Bacon (2021) is only one of the many developments emerging from the active econometric methods research on

³⁶Note that since the three other factors that we cannot individually estimate work in opposition (two would stimulate crime and one would suppress it), their individual effects could be meaningful, even if their combined influence was negligible. Using our elasticity calculations in footnote 38, above, we would surmise that the combined effect of all other factors, including these three and possibly others we have not addressed, would be to increase violent crime by about one-third of the total 13 percent increase that flows from RTC adoption.

staggered-adoption panel data analyses. Further research will likely refine the best econometric techniques in this domain.

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